

pressibility of elements, amongst other facts, it is evidently possible to influence intra-atomic activity by means of forces applied from without. It seems probable, therefore, that pressure, if sufficiently great, could be transmitted to the interior of a uranium atom and supply the countervailing force required to help the atom past a position of instability. Once we realise this probability, we see a cogent reason why the interior of the earth should be free from radium and all other radio-active substances the formation of which from heavier elements demands an increase in volume, these heavy elements being at great depths only potentially radio-active.

From this point of view radio-activity is, as Dr. Schiller suggests, "an acquired habit of the substances that exhibit it," the habit being acquired when such an element passes by any means whatever from great depths below the earth's surface to regions of less pressure.

In a paper just issued in the Records of the Geological Survey of India (vol. xliii., part 1) I have given a preliminary account of these speculations concerning the mineralogical constitution of the earth's crust, and in a final paragraph have appended a brief reference to this question of the inhibition of the disintegration of uranium at great depths below the earth's surface.

L. L. FERMOR.

Pianoforte Touch.

IN connection with the discussion on this subject originated by Prof. Bryan's paper, I may mention that I have been making some measurements during the past winter with the view of obtaining some idea of the velocities and forces involved in the motion of the transmitting mechanism. By fitting an electric chronographic arrangement to an upright piano I have been able to measure the actual times taken in different stages of the movement with different degrees of loudness.

Among other results, I have found that the time during which the hammer is flying freely towards the string after losing contact with the propelling mechanism varies from about 0.04 sec. for *pp* to 0.001 sec. for *ff*. The distance of flight was about 1 cm., so the velocity ranged from 25 to 1000 cm. per sec. At the latter extreme, however, the force used in striking the key was greater than would be used in ordinary playing. On the other hand, when a weight was allowed to fall on the key from the smallest height to produce a note the time could be brought up to 0.07 sec., corresponding to a velocity of only 14 cm. per sec.

With regard to the much disputed point as to whether it is possible to vary the quality of the note independently of the loudness, this must, I think, still be regarded as an open question. Very positive affirmations are made by musicians on both sides. From a physical point of view the suggestion made by Mr. Tobias Matthay in his work on touch seems to be the only possible way to explain the effect, if it exists, viz. that the quality can be spoiled by vibrations of the hammer-shaft at the instant of striking the string. In accordance with this, Mr. Matthay holds that the tone is good when the final velocity is given to the key gradually, and is bad when the same velocity is imparted by a sudden blow. That vibrations of the shaft occur is, of course, undoubted, but whether they are sufficient to cause an appreciable effect is another matter. On the other side, recent German theorists, such as Breithaupt, Steinhäuser, and Ritschl, deny the effect altogether. The last-named author holds that good touch consists in the power to produce fine gradations of intensity and in complete mastery of *legato* and the use of the pedal.

The essential question seems to be whether good and bad touch can be distinguished in a *single note* struck and allowed to die away, or in a succession of notes following each other at so long intervals as to be musically detached. As I understand Prof. Bryan, the improvement effected by his invention is chiefly apparent in a sequence of notes forming a melodic phrase. Further, I think that the exact dynamical effect of the contrivance on the motion of the key has not been made quite clear in the published account.

W. B. MORTON.

The Queen's University of Belfast, July 2.

The Reflection of X-Rays by Crystals.

IT is interesting to find that an X-ray bulb having a rhodium antikatode gives off a strong, sharply defined (and therefore very homogeneous) beam which is reflected from the (100) face of rock-salt at a glancing angle of 6.2° . Its mass absorption coefficient in aluminium is 3.2. A second weaker beam is reflected at an angle of 5.8° , and this appears to complete the rhodium X-spectrum. Assuming the correctness of my son's determination of the spacing of the atoms of rock-salt (in a paper read before the Royal Society on June 26), the wave-length of the stronger beam is 0.61×10^{-8} , and of the weaker 0.57×10^{-8} . It can be calculated that radiation of about this wave-length should be emitted by a rhodium antikatode; the argument is given in a paper recently read before the Royal Society (see abstract on p. 496 of this issue).

Platinum and rhodium give much stronger homogeneous reflected rays than iridium, tungsten, or nickel. The current produced in an ionisation chamber 15 cm. long, filled with SO_2 , the slits being 3 mm. long and 0.8 mm. wide (Proc. Roy. Soc., lxxxviii., p. 428), is so great that the leaf of a Wilson electroscope races across the field of view at the rate of thirty or forty measurable divisions (three or four scale divisions) in a second. The setting must be exact, and the bulb should be very soft.

W. H. BRAGG.

Rosehurst, Grosvenor Road, Leeds.

Wireless Antennæ.

PROF. FLEMING, in a recent letter to a contemporary journal, has made a suggestion similar to that of Mr. A. Lander, in NATURE of July 3 (p. 451), to the effect that the space wave in wireless telegraphy is supplemented by some effect which travels through the earth. Indeed, it would appear natural to expect that, in addition to the electric disturbance which must travel outwards in all directions over the conducting surface of the earth when the electrical potential at any point on this surface is disturbed, the passage of the electromagnetic waves through the air above the earth's surface should be accompanied by some form of electrical disturbance along the conducting earth's surface beneath them. This is, however, a subject that, so far as I am aware, has not yet been tackled by mathematical physicists, and I would point out that it is well worth their attention.

In connection with the matter, it may be of interest to mention that I find that my own body, without any wires or anything else, will serve as antenna for the reception of signals from the Admiralty. My receiving apparatus is on the ground floor of my house in Chester Square, and with my ordinary aerial disconnected I find I can get the Admiralty signals, faintly but quite audibly, simply by touching with my finger the terminal to which the aerial is usually connected. No doubt in this case my body does not act as an aerial in the ordinary way, but merely as a capacity into which the electrical disturbance arriving

through the earth passes in and out through the receiving apparatus. The Admiralty station is, of course, comparatively near, and the signals are very powerful. This explains why it is only Admiralty signals that I am able to receive by this method.

I am unable to agree with Mr. Lander in his remarks as regards tuning, as I find that with my bedstead aerial it is just as easy to tune in and out such signals as I am able to receive as it is with my proper aerial, which is suspended on poles above the roof of the house. The Eiffel Tower signals are always difficult to tune out, for the reason, as I suppose, that they are of irregular wave-length, while I find it impossible to tune out the Admiralty by reason of its power and proximity. Norddeich and other unidentified signals that I obtain are, however, tuned in and out both with the bedstead aerial and with the other with equal facility.

For time signals very accurately tuned waves, such as are sent out by Norddeich, are perhaps not altogether an advantage, as badly tuned waves, such as are sent out by the Eiffel Tower, are much more easily picked up by all and sundry.

A. A. CAMPBELL SWINTON.

66 Victoria Street, Westminster, S.W., July 7.

A Mechanical Vacuum-Tube Regulator.

MAY I supplement Mr. Campbell Swinton's letter in NATURE of June 26? The device of sliding a glass sleeve over the kathode for the purpose of varying the hardness of a discharge tube was also used and fully described by Wehnelt in 1903 (*Deutsch. Phys. Gesell. Verh.*, 5, 14, p. 259), some five years after Mr. Swinton.

The important part that the electrification on the walls plays in a discharge tube is not, I think, generally realised; and Mr. Swinton is not quite right in assuming that Mr. Whiddington's explanation is novel.

The electrification on the glass walls adjoining the kathode, and its concentrative effect on the beam of kathode rays, were remarked by Goldstein in 1901 (*ibid.* 3, 15, p. 192).

I remember some half-dozen years ago, Sir J. J. Thomson, in one of his lectures at Cambridge, gave a similar explanation of the formation of the fine pencil of kathode rays which can be seen crossing the bulb from the centre of the kathode in a soft X-ray tube. He attributed the effect entirely to the negative electrification of the glass round the kathode. The pencil of rays is as definite with a plane kathode as with a concave one.

But X-ray tube-makers have long been aware that, by withdrawing the kathode from the bulb into a side tube, the discharge can be hardened. In the earliest X-ray bulbs, the kathode was always mounted in the body of the bulb; but the advantages of a side tube had been realised by 1896, and the design has since been universally adopted.

Mr. Swinton was also responsible about 1897 for another adjustable form of X-ray bulb, in which, instead of a sliding sleeve, a movable kathode could be advanced in or out of a side tube. The bulb is at present in the Röntgen's Society's historical collection in the South Kensington Museum.

G. W. C. KAYE.

June 28.

In order to remove the possibility of any misunderstanding that may arise from Mr. A. Campbell Swinton's letter in NATURE of June 26 (p. 425), may I state that the mechanical vacuum-tube regulator is *not* claimed by me as new in the paper referred to. If Mr. Campbell Swinton will read the actual paper he

will find it clearly stated that the regulator was discovered by him.

RICHARD WHIDDINGTON.

St. John's College, Cambridge, July 7.

The Humphrey Owen Jones Memorial Fund.

THE committee formed to carry out the generally expressed desire that some suitable memorial of the late Humphrey Owen Jones, F.R.S., should be established, has received subscriptions amounting to about 3600l. It is proposed to devote the sum collected to the endowment of a teaching post in physical chemistry in the University of Cambridge.

The committee desires to close the subscription list at the end of this month, and requests further intending subscribers to send their contributions to the account of the H. O. Jones Memorial Fund, Messrs. Barclay and Co.'s Bank, Cambridge, before that date.

W. J. POPE

(Chairman of the Committee).

The Chemical Laboratory, Cambridge, July 7.

Smithsonian Physical Tables.

ATTENTION was directed by Mr. C. T. Whitmell on p. 320 of NATURE of May 29, to a "very awkward error" in the Smithsonian Physical Tables (1896). The institution is always glad to have attention directed to errors for correction in subsequent editions, but as this particular error does not appear in the first revised, second, third, fourth, and fifth editions, it seems rather unfortunate to have discredit thrown on the tables through an error long since corrected.

C. D. WALCOTT,

Secretary.

Smithsonian Institution, Washington, U.S.A.,

June 21.

MODERN VIEWS OF ELECTRO-THERAPEUTICS.

DURING the last few years our views upon the true meaning of the action of electricity upon living subjects have been growing much clearer. We begin to see the principles upon which our practice should be based, and already, as a consequence of this, our methods are changing and our results are growing more valuable.

There are two factors which have brought this about. One is the recognition of the importance of the theory of ions in all matters which concern the movement of electric currents in living tissues, and is due to the genius of Leduc, and the other is the recognition of the thermal action of high-frequency currents, an action which remained unappreciated, even if not unknown, until it was insisted on and emphasised by Nagelschmidt. It is upon these two basic facts, the chemical or ionic effects and the thermal effects of electric currents, that the electro-therapeutics of the future will be established.

First, as to the chemical aspect of the medical applications of electric currents. To begin with, all movements of current in the body, whether the currents are direct, interrupted or alternating, are ionic movements pure and simple, and their effects are due to the chemical displacements produced. We may not speak of effects which are additional to or independent of the ionic movement, for such effects do not exist. The current in the body is the double ionic movement only. The treatment